Reviewers' comments (Oct. 11th, 2017)

Reviewer #1: Dear Author, please find here a list of comments from my side.

Summary: the article describes the layout, operation and performances of a Radial Time Projection Chamber with the electron amplification stage provided by three layers of GEM. The main novelty is the application of the electron multiplier for the amplification stage in a TPC on a semi-cylindrical shaped plane. The detector, originally built for the BoNuS experiment, is in fact formed by two semi-cylindrically shaped halves connected together, with the axis coincident with the beam line.

The detector was implemented to measure DVCS of electrons on different light nuclei and the performances were evaluated on electron-^{4}He elastic scattering runs.

Chapter 1 describes the CLAS@JLab detector, inside which the RTPC was installed and took data. Chapter 2 focuses on the RTPC mechanical and electrical layout. From inner to outer, there are: the ^{4}He gas target, a ^{4}He gas gap, the actual time projection chamber. The time projection chamber is a 200 mm long detector composed by different gas gaps limited by concentric electrodes around the axis: cathode (R= 30 mm), G1(R= 60 mm), G2(R= 63 mm), G3(R= 66 mm), readout plane (R= 69 mm). Everything fits inside a 5T solenoidal magnetic field. It provides both time and charge measurements. Chapter 3 is a short description of the electronics and readout, mainly inherited from ALICE and BoNuS. The improvement stands in the higher readout rate capability. Chapter 4 talks about calibration of drift velocity, drift paths and energy loss measurements by the usage of simulations with MAGBOLTZ and tuning the results on elastic scattering data. Chapter 5 concentrates on track finding/fitting procedure and noise evaluation and subtraction. Chapter 6 reports on resolution and efficiency from the elastic scattering reconstruction of 1 GeV electrons on 4He and Chapter 7 concludes the paper.

The article main points of interest are the followings:

a) The usage of GEMs as avalanche creators inside the time projection chamber. As far as I know other TPC with the wires substituted by GEM layers have been (or are being) realized, like ALICE TPC upgrade or the FOPI one (proposed for PANDA), but in those cases the GEM are planar and on the end plane of the TPC, not around the beamline.

b) The shaping of the GEM foils around the target. However, since now a fully cylindrical GEM foil has been realized (for the KLOE-2 detector and for the next BESIII inner tracker) I would not call these "cylindrical" GEMs, but "semi-cylindrical" GEMs to point out the difference (which reflects in the construction procedure). Maybe these other experiments using GEMs should be mentioned due to the similarities, thought they are successive in time to this RTPC construction.

Concerning the work description, it is mainly exhaustive, but in some points there are open questions that I would like to submit to the authors in the following.

Chapter by chapter comments

Chapter 2

1. As already said I would not speak of cylindrical GEMs but semi-cylindrical. Moreover, please add the reference to Sauli's paper [11] when speaking the first time about GEM in the text.
   cylinderical GEMs-> semi-cylinderical. The reference has been added.

2. In my opinion, this chapter should highlight better which are the improvements w.r.t. BoNuS detector to underline the originality of this work. For example, better acceptance? Higher gain? Higher electric stability? Lower material budget? Please quote some old and new values, maybe in a table, to make them evident.
   To the group: Do we have such a comparison?

3. Relevant length and thicknesses of the various elements are reported in the text and in the figure, can you please add also the z position of the target w.r.t the detector? To give the complete information, I would also add the value of the induction gap electric field, which is the only missing field value and the value of the reached amplification gain with the three GEM.
   The text has been modified to clarify the z position of the target with respect to the detector.
   - Regarding the electric field, it was 2.8 kV/cm in BoNuS, does anyone know our exact field in this gap?
   - The gain of such GEM configuration has been studied by CERN's Gas Detector Development Group (Fabio Sauli, Progress with the Gas Electron Multiplier, 2nd Workshop on Advanced Transition Radiation Detectors for Accelerator and Space Applications (Bari, Sept. 4-7, 2003), Nucl. Instr. and Meth. A522(2004)93). The results, see the reference, demonstrate that one should not simply add gains to each other for multilayer GEM amplifications. We did not evaluate precisely the overall gain in our chamber, so we do not have a value for our RTPC GEM system at this point.

4. When speaking of low spark rate the reference [12] is quoted, which is the PDG. Maybe it is better to quote more specific studies like the ones by Bachmann et al.
   Thanks. It has been replaced.

5. line 119: is the Kapton of the GEM foil really 300 mum thick? It is usually very thin, like 50 mum, in order to have low material budget and very high fields.
   Sorry typo. It is 50 mum

Chapter 3

6. The description of the electronics looks to me a little not well organized, like a collection of info from previous descriptions from ALICE and BoNuS. It is a series of details, some of which not really relevant for the good understanding of the work described in the paper. So, I would put only some general info (the number of channels/pad, the number of time samples and their width for each channel and the number of ADC samples and their width for each channel) with the reference where to find more material and just write explicitly the novelties, such as the fact that the acquisition rate was increased (footnote 2).
   Raphael, Can you check here?

7. Also, I understand that fig. 5 comes from reference [14], updated with the values of the RTPC instead of the ALICE-TPC, but it seems to me that there are some parts of the picture which have to be fixed. I list them here:
- why does it say 768 pads?
- why 7.6 microsec?
- the anode wire e grid make no sense here
- why L1 and L2 (trigger levels?)
Raphael, Can you check here?

Chapter 4

/paragraph 4.1

8. I would really prefer to call it drift "velocity" instead of drift "speed"
To the group: We do have radial and azimuthal drift velocities, but we measure the average of their sum.

9. I don't really understand the procedure to find the drift velocity described here and fig. 7. What is Nb_{max} and $Nb_{max/2}$? Why does $T_{max/2}$ correspond to $Nb_{max/2}$ and why is it used as T_{max}? If I make a quick calculation and use T_{max} 65 * 100ns and a drift length of 3cm /cos(23Â°) 7.5 cm I get a v_{drift} 1.15 cm/microsec « 5 cm/microsec which is the average value of drift velocity in drift chambers.
The procedure according to me would be:
- for each hit associated to a track, whose time information is available, take the drift time t_{drift_i};
- plot the distribution of these t_{drift_i};
- find the T_{min} = minimum t_{drift_i} and T_{max} = maximum t_{drift_i}
- compute $delta_t = T_{max} - T_{min}$ and extract the drift velocity by dividing the track length / $delta_t$. Figure 8 would have the $delta_t$ instead of the T_{max} vs z.
Please provide more details on the applied procedure, since I have the open questions I wrote before and I could not understand it properly. Moreover, please quote the value of the found drift velocity. Is it in accordance with the simulations?

10. Figure 6: why are T_{min} and T_{max} not at the drift gap extremities?
The figure has been updated.

/paragraph 4.2

11. line 306: "in one bin" should be explained better. I understood it is the subdivision in bins of the z coordinate, but it was not so obvious at the first reading
The text is cleared

12. Has the stability of the drift path been counter checked with simulations varying the field according to the cathode radius variation? How much is the field variation if the radius have some mm of uncertainty?
We did not study this case.

/paragraph 4.3

13. The gain calibration is very quick. I would like more details, e.g. "setting the simulation properly" intends only GEANT4 or also MAGBOLTZ or GARFIELD? Were they used to

simulate the drift and avalanche formation of the electrons? And the diffusion from the gas? Was a specific code written to simulate the electronics?
Only GEANT4 simulation was used in performing the gain calibration, where the drift paths are updated in the simulation. The electrons avalanche formation and the gas diffusion are performed though GEANT4. We did not implement a detailed description to simulate the electronics, but we did apply similar conditions to match the real DAQ, such as the rejection of the signals from bad pads and conditions to ensure that each pad must have at least 3 consecutive time bins, with ADC values above the threshold, 35 ADCs, in order to be recorded. The text is cleared with more details.

14. line 330: "correction factors" are mentioned. On what? Why haven't only good tracks been used for calibration? Lack of statistics?
Correction factor on the extracted gain for each pad. We used the elastic ^{4}He tracks as we could calculate the recoil ^{4}He kinematics from the scattered electron in CLAS to avoid any discrepancy from the RTPC.

15. When it says that the pad charge is "compared" to the neighboring ones, what happens next? Is it the noise reduction step described later? If so, please write it.
The gain extraction is following this order:
- pass 1: the gain of each pad is defined as the ratio between the mean recorded experimental ADCs to the mean simulated ADCs in the same track. Then for each pad, the gain ratios were collected from all the identified elastic tracks and these gains were fitted by a Landau function to give a first pass gain for each pad as the most probable value of the fit.
- pass2: to make sure that the recorded ADCs by a given pad are similar to the recorded ADCs by other pads in the same track, we refined this calibration after the extracted gains were applied to the experimental data. At this step, we compared the mean ADCs of each pad to the mean ADCs of the whole track. This ratio is collected from all the elastic events and a gain correction factor is extracted and applied giving a final gain for each pad.

16. Figure 11: I don't understand it. How many tracks does the histo contain? If the single pad has 128 channels, how many channels does the single bin contain? Moreover, is the dE calculated on pad by pad basis? In that case, why don't you put just one point in the histo for each pad? What is the mean number of channels or pads fired by one track? Sorry for the list of questions, but I could not understand the figure.
- It is for only one track (written in the caption). - We dont understand your second question. A pad is a channel. Do you mean how many pads are fired for a single track? Figure **??** shows the number of active pads as a function of the momentum for the collected elastic ^{4}He tracks.
- dE is calculated from the ADCs of the individual pads that read out hits corresponding to the same track. One needed to know the individual step size very precisely to put dEdx from the individual pads, which is not available. - Last question is addressed by figure **??**. The mean value is around 10 pads.

17. Figure 12 - please, put it after figure 11. Moreover, please change dEdx to dE/dx on y axis. Why are the left and right pictures so different? Talking about the deuteron band which is present only in the left figure and the smear on the top part of the right picture.
- The figures layout will be updated.

- The y-axis label on figure 12 is updated.
- Regarding the lower peak in the left module of the RTPC, these events pass all the elastic

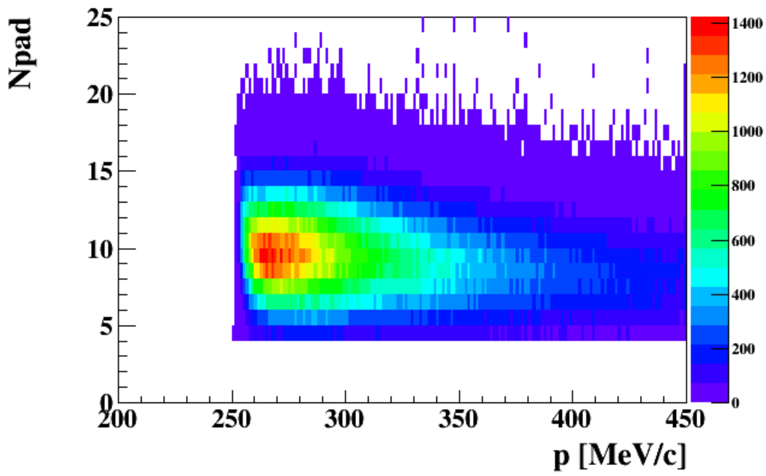

Figure 1: The number of active pads versus the measured momentum of the elastic ^{4}He tracks.

requirements but for some reasons they have lower ADC values. They represent around 7% of all the elastic events. After extended studies, the nature of these particles is not identified yet. We note that this is a global phenomenon in the left module as 94% of the left module's pads are involved in both some low and high dEdx events. For the calibration procedures, the events with low dEdx were excluded as we do not fully understand their nature.

Chapter 5

/paragraph 5.1

18. Please detail a little bit more the procedure to remove the oscillatory noise. It says "event by event and channel by channel": if the ADC threshold is passed then a noise level is subtracted? If so, how is it calculated?
Nathan, Can you work on it.

/paragraph 5.2

19. How was "10.5 mm" cut chosen? is it in xy plane or in 3-dimensions?
The distance between hits are calculated in 3D. This cut has been chosen because it enables us to jump longer gaps from dead areas of the readout pads without increasing the level of spurious tracks.

20. How was the "10 hits" cut chosen? What is the mean number of primary ionization in this gas mixture?
Figure 2 shows the number of hits distribution for the collected elastic ^{4}He tracks from the 1.2 GeV dataset. The mean number of hits is around 135 and the 10 hits cut at the reconstruction level is a loose cut to reduce the level of the spurious tracks.

21. $< dE/dx >$ is computed here as corrected Etot divided total track length. Usually the mean dE/dx is computed with the truncated mean of the specific energy losses of each hit. Isn't it possible to compute it like this here? dx for each hit can be (maybe) computed from the difference in the hit times. Sum dE_i/Sum dx_i is not equal to Sum (dE_i/dx_i).

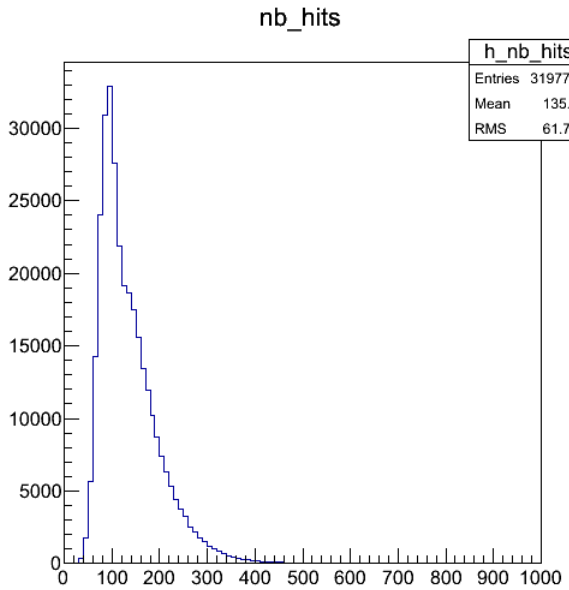

Figure 2: the number of hits distribution for the collected elastic ^{4}He tracks from the 1.2 GeV dataset.

Maybe this would improve our calculations for dEdx by few percent, but a precise hits ordering is needed to compute dEdx as you suggest, which is not available in our setting.

Chapter 6

/paragraph 6.2

22. I understand that efficiency is a ratio between two integrals, each integral being the area of the Gaussian which fits the recoil mass distribution in the inclusive and exclusive case. If so, would it be possible to please draw both fits on the figure 14 and provide the integrals to compute the mean efficiency? If I misunderstood, please comment.
Nathan, do you still have the individual integrals?

Chapter 7

23. Is it "high rate environment" or "high readout rate"? They are connected, but what is the rate the RTPC underwent during data taking?
Can we find such information in the logbook?

24. I understand the resolution and efficiency have been evaluated with the 2 GeV electrons. Is it foreseen to analyze also the 6 GeV electron runs, which were the actual data taking? Is there an evaluation of the foreseen RTPC performances in that case?
Typically, the elastic reaction is used for such performance evaluation. But the elastic cross sections is really small at 6 GeV beam energy.

Some general remarks on the layout

25. I would add to the keywords also GEM or gas electron multipliers or multi pattern gas detectors (it depends on what is available).
    Updated
26. Please put the units on the axis of every figure, together with the physical quantity. In some figures it is only written in the caption or in the text.
27. Sometimes to explain the dimension of something something like "250 mm long" (line 67) while some other times "84-mm-long" (on line 83) is written. Please make this uniform in the whole text (I just quoted two examples, but there are more in the text).
    Updated everywhere
28. In footnote 2, BoNuS is written BONUS and somewhere else in the text also BoNus. Please uniform this to the correct one, which I think is BoNuS.
    Updated to BoNuS everywhere
29. Please make the bibliography entries uniform: they are not all written with the same layout.
    Updated

    List of in text corrections:
30. line 17: "GeV" –> "GeV/c"
    Corrected
31. line 26: "nuclei" –> "nucleus"
    Corrected
32. line 40: "to track" –> "to bend tracks"(since the magnetic field just bends the track and then the detector tracks them)
    Added
33. line 91: "second gap" –> "second gas gap"
    Added
34. line 110: "from the GEMs" –> something like "after they have been multiplied by the GEMs" or "induced in the last gas gap" since the GEM is the amplification system.
    Added
35. line 131: "axial magnetic field" –> "solenoidal magnetic field"
    Replaced
36. line 133: "challenge" –> "challenges"
    Added
37. line 133-134: "radial TPC" it would be better to uniform the way to write it in the whole text (like, "RTPC")
    Uniformed
38. line 167: "stage" –> "stages"
    Corrected

39. line 176: "ReadOut" should always be written in the same way (see line 168) unless this was specifically written in a different way in the cited articles
Uniformed

40. line 208: cm^{-1} . s^{-1} –> cm^{-1} cdot s^{-1} (the dot is not in the center vertically)
Corrected

41. line 208 again: "6.067" before it said "6.064", please fix the wrong one Corrected. It is 6.064

42. line 263: "2%" –> I would put "around 2%"
Added

43. line 268: "reconstructions" –> "reconstruction"
Corrected

44. line 269: "Paths" –> "Path"
Corrected

45. line 308: "codes" –> "code"
Corrected

46. line 405: "electron's" –> "electron"
Corrected

47. fig14 - caption: "W distribution" –> I would put "recoil mass W distribution"
Added

48. table 1: $sigma_p$ is actually $sigma_{p/p}$, since it is in percentage
Yes it is. Corrected

49. line 432: "Helium-4" –> "4He" (for uniformity, as was written before)
Changed to ^{4}He

50. line 488: There is an extra "," at the beginning of the item
Cleaned

The topic of the article is interesting but I would recommend a revision. Please I would like the authors to address the questions asked and provide the required changes or explain why they think those changes are not necessary/correct. Thank you. Best regards.